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ABSTRACT

The recession of the 1970s eroded equal employment opportunity gains made under Title VII of the Civil Rights Act of 1964. The unemployment rate during the early 1970s interacted with pre-amendment Title VII settlements to limit the increase in female earnings, exaggerate the reduction in male earnings, and restrict the narrowing of the earnings differential. Nevertheless, even with high unemployment rates, Title VII still narrowed the sex differential in earnings. But the real earnings cost was greater and the earnings redistribution less than at a lower employment rate. (Author/EB)

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THE EFFECT OF ECONOMIC CONDITIONS ON THE SUCCESS OF EQUAL EMPLOYMENT OPPORTUNITY LAWS: AN APPLICATION TO THE SEX DIFFERENTIAL IN EARNINGS

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The Effect of Economic Conditions on the Success of Equal Employment Opportunity Laws: An Application to the Sex Differential in Earnings

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ABSTRACT

Because compliance with Equal Employment Opportunity laws costs firms more in a recession, enforcement will improve the relative position of women by less then. Moreover, previous gains from enforcement may be eroded as the old adage "last hired, first fired" takes hold. Indeed, we show empirically that Title VII of the Civil Rights Act of 1964 could have increased female earnings and narrowed the sex differential by more than it did were it not for the recession of the early 1970s. The data suggest that the recession did not preclude new wage gains but did erode gains made during the late 1960s.

The Effect of Economic Conditions on the Success of Equal Employment Opportunity Laws: An Application to the Sex Differential in Earnings

Worsening economic conditions curtailed the effectiveness of equal employment opportunity (EEO) laws during the early 1970s. But for the recession of the 1970s, Title VII of the 1964 Civil Rights Act would have increased female earnings and narrowed the sex differential by more than it actually did. This paper empirically distinguishes the effects of Title VII itself from those induced by the economic environment.[1]

Overall between 1968 and 1974, enforcement of Title VII increased female earnings and narrowed the sex differential; but, despite overall gains, enforcement occurring prior to March 1972 had reduced the earnings of both sexes by 1974 (Beller, forthcoming). Although negative long-run EEO effects are consistent with certain microeconomic predictions (Heckman and Wolpin, 1976)[2], the coincidence of much of this six-year period with a cyclical decline in economic activity suggested that those effects might have been caused by macroeconomic conditions.[3]

A recession can erode gains and impede progress toward EEO goals by increasing the monetary and psychic costs that firms complying with EEO laws face. If such costs vary inversely with cyclical changes in economic activity, then we should find a pro-cyclical pattern in firm compliance. Compliance by firms also depends directly upon the expected costs of violating the law. Firms' costs vary with their (1) probability of getting caught violating the law and (2) probability of paying a penalty once caught. In 1972, amendments to Title VII increased these expected costs; a discrete

change in the effects of these probabilities in 1972 can capture the amendments' impact.[4] A continuous change in the effects of these probabilities over time can capture the interaction between compliance costs and the business cycle. Upon this framework, we can build an econometric model. Its estimates show the effect of Title VII on earnings in 1968 through 1974 to be inversely related to the aggregate unemployment rate. These results confirm that macroeconomic conditions can alter the effects of social programs, a fact which analysts must take into account.

1. THE EFFECT OF ECONOMIC CONDITIONS ON COMPLIANCE WITH EEO LAWS

As aggregate economic conditions worsen, the cost to firms of compliance with EEO laws increases for two reasons. First, in a recession, both the demand for labor and voluntary turnover are reduced, costing the firm more to attain a given increase in its female/male employment ratio. And second, more intense competition for fewer jobs makes male workers and unions more likely to resist the hiring and promotion of women workers.

At full employment increased demand creates new jobs and new vacancies when employees leave for better jobs. To comply with EEO laws, firms can then increase their employment ratio of women to men by including a high proportion of women among the new hires. In a recession new jobs are created more slowly and individuals change jobs less frequently. Firms then find it harder to increase the female/male employment ratio; they can fill all job vacancies with women, further reducing the number of positions available to men or resort to layoffs. Laying off a worker means that the firm loses its investment in him, whether it includes only fixed hiring

costs or hiring and training costs. If firms first lay off workers in whom they have the smallest investment, then, as the recession deepens, they incur an increasing loss by replacing a current (male) employee with a new (female) employee. Clearly, the weaker the economy the more it will cost firms to increase the female/male employment ratio.

Not only will the same increase in employment ratio cost employers more, but also male employees and labor market institutions such as unions will resist it more. The fewer his alternative opportunities, the more likely a male worker will resist losing a job to a female. This explains the recent increase in charges of reverse discrimination. Many male workers, however, need not act on their own; unions and seniority systems impose institutional constraints on hiring and promoting women, particularly new entrants. Furthermore, these constraints are more likely to be binding during a recession.

Economic conditions will affect the long-run as well as the short-run impact that enforcing EEO laws can have on the position of minorities and women. Employers first lay off workers in whom they have the smallest investment, usually new hires. Employee resistance, unions, and seniority systems also protect the position of older employees. Thus, the last hired in a prosperous economy will be the first fired when economic conditions worsen.

If compliance with the law costs more when economic conditions weaken, then a given amount of enforcement will induce less compliance. Both the scope of settlements reached and the extent to which firms adhere to them will be reduced. Consequently, enforcing EEO laws will increase the demand for women relative to men by less in a recession, and will increase female earnings and narrow the earnings differential by less as well.

Even if the extent of compliance were not reduced, enforcing EEO laws in a recession would increase earnings by less (or reduce them by more). In the attendant loose labor market there are fewer employment opportunities. Because firms need not bid women away from other alternatives, they can increase their female/male employment ratio without increasing female wages. And a given increase in that ratio is more likely to displace male workers, tending to reduce male wages by more than in a tight labor market.

In summary, the impact of enforcing Title VII is hypothesized to depend upon aggregate economic conditions because the costs of complying with the law will vary inversely with cyclical changes in economic activity. Fewer firms will comply with Title VII when it costs more to do so; for those that do comply, the scope of and degree of adherence to conciliated and litigated settlements will be reduced. Thus, a given level of enforcement will increase the demand for women relative to men by less in a recession than otherwise, tending to increase the wages of women by less. For a given increase in relative demand, the increase in women's wages is further limited because there are few alternative employment opportunities. Because there are fewer opportunities, enforcement is also more likely to displace men from jobs than it would at full employment. As a result, enforcing Title VII is expected to increase female wages by less and to reduce male wages by more during a recession than at full employment.

2. AN ESTIMATION MODEL

In this section, we develop an econometric model to test the

impact of cyclical variation in economic activity on year to year differences in the effect of Title VII on the sex differential in earnings. To estimate the effects from 1968 through 1974, the model pools time series and cross sections and allows unrestricted year coefficients on the Title VII enforcement variables. Then to estimate the impact of cyclical variation in economic activity on these yearly effects, the model restricts them to a linear function of the inverse of the aggregate unemployment rate. By using that rate to measure the aggregate level of economic activity, we follow the convention established by previous studies (e.g., Ashenfelter, 1970; Vroman, 1975).

The unrestricted (1) and the restricted (2) versions of the model may be written as follows:

$$\ln W = \alpha_o + \alpha_1 U_t + \sum_j \sum_t \beta_{jt} E_j + \sum_i \sum_t \gamma_{it} X_i + \sum_k \delta_k Y_k + \epsilon \qquad (1)$$

$$\ln W = \alpha_o + \alpha_1 U_t + \sum_j (\beta_{1j} + \beta_{2j}\, 1/U_t) E_j + \sum_i \sum_t \gamma_{it} X_i + \sum_k \delta_k Y_k + \epsilon \qquad (2)$$

where,

$\ln W$ = natural logarithm of real weekly earnings for individual, I

U_t = aggregate unemployment rate in year t, where t is the year in which individual I appears in the sample

E_j = a vector of j enforcement variables assigned to individual I on the basis of geographic location and class of worker

X_i = a vector of i human capital variables for individual I, the coefficients of which are allowed to vary over the t years

Y_k = a vector of k control variables for individual I

β, γ, δ = vectors of parameters on the enforcement, human capital, and control variables, respectively

and ε = a disturbance term with the classical properties.

Specified as human capital earnings functions, these equations allow the coefficients on the major human capital variables, such as education and experience, and on the EEO variables to vary across years and constrain the coefficients on the control variables to be the same in all years.[5] Separate equations are estimated for males and for females, allowing sex to interact with all of the parameters. The variables in the equations are listed in the Appendix, Table A.2.

The four enforcement variables measure the pre- and post-amendment probability of apprehension for violating Title VII and probability of paying a penalty if found violating it; unlike the other variables in the equations, each of them varies cross-sectionally but not over time. The probability of apprehension is estimated by the ratio of the number of investigations of sex discrimination charges completed by the Equal Employment Opportunity Commission (EEOC)--or by the state or local Fair Employment Commission to which a charge has been deferred--to the number of women in each state group and of each class of worker who worked during 1970. The probability of paying a penalty is estimated by the ratio of successful settlements of sex discrimination charges (successful conciliations plus successful predecision settlements) to attempted settlements. Each pre-amendment enforcement measure is computed from data from January 1968 through March 1972, and each post-amendment enforcement measure from April 1972 through December 1974.

We test the following hypotheses,

H_0: The aggregate unemployment rate does not account for all of the year to year differences in the effect of enforcement on earnings.

H_0^*: The aggregate unemployment rate accounts for none of the year to year differences in the effect of enforcement on earnings.

against the alternative hypotheses,

H_1: The aggregate unemployment rate accounts for <u>all</u> of the year to year differences in the effect of enforcement on earnings.

H_1^*: The aggregate unemployment rate accounts for <u>some</u> of the year to year differences in the effect of enforcement on earnings: The higher the unemployment rate, the less positive the effect.

To test H_0 against H_1, whether or not the unemployment rate accounts for <u>all</u> of the year to year differences, we compute an F-test by subtracting the error sum of squares of equation (2) from that of equation (1). In order to test H_1^* against H_0^*, whether or not the unemployment rate accounts for <u>some</u> of the year to year differences, we need unbiased estimates of the interaction between enforcement and the unemployment rate. But the effect of enforcement is expected to grow over time, and the unemployment rate trended upward during the period studied; hence, their interaction could pick up that common trend. Thus to avoid spurious estimates, we add a time trend to the enforcement variables, rewriting the restricted equation as follows:

$$\ln W = \alpha_o + \alpha_1 U_t + \sum_j (\beta_{1j} + \beta_{2j}\, 1/U_t)\, E_j + \sum_m \beta_{3m}\, t_1\, E72_m + \sum_n \beta_{4n}\, t_2\, E74_n + \sum_i \sum_t \gamma_{it} X_i + \sum_k \delta_k Y_k + \varepsilon \qquad (3)$$

where

$t_1 = 1, 2, \ldots, 7$

$t_2 = 1, 2, 3$

$E72_m$ = two pre-amendment enforcement variables,

and $E74_n$ = two post-amendment enforcement variables.

To test H_1^* against H_0^*, we can then test whether $\beta_2 > 0$. We can also test the hypotheses $\beta_3 > 0$ and $\beta_4 > 0$, that the effect of Title VII's enforcement grows over time.

The effect of Title VII should grow over time for the following reasons. First, according to Bayesian theory firms increase the weight given to sample information over time. They use information about enforcement to modify their prior probability. Putting more weight on such information should increase the firms' response to enforcement, causing the coefficients on the enforcement variables to increase over time. Second, even as firms put more weight on sample information, an adjustment lag may still occur, reinforcing the positive trend on effects of enforcement. Third, if discrimination against women had diminished, reducing the costs of complying with Title VII during the time period studied, it would have increased the effect of enforcement. Finally, because all of the enforcement variables include several years of data, they may be measured with error in the earliest years of the sample, biasing those estimated coefficients toward zero.

The Annual Demographic File from the U.S. Census Bureau's 1969-1975 Current Population Surveys (CPS) provide data for the earnings, human capital, and control variables. The samples include all white men and women who worked, other than the self-employed, and who earned at least $100 in wage and salary income.[6] Since these individuals differ from year to year, the

data are not longitudinal. The enforcement variables are defined to coincide with the 23 state groups identified in the CPS for the private and government sectors separately; each individual is assigned a value according to their state group and class of worker. The mean investigations per 1000 employed women is .109 pre-amendment and .310 post-amendment. The mean probability of successful settlement is .508 pre-amendment and .542 post-amendment. These data were obtained from the compliance files of the EEOC.

3. EMPIRICAL RESULTS

Table 1 presents coefficients on the enforcement of Title VII estimated by unrestricted equation (1). The coefficients illustrate the year to year differences in the effect of pre-amendment enforcement on earnings by sex from 1968 through 1974, and the effect of post-amendment enforcement from 1972 through 1974. The table also presents coefficients of enforcement on earnings in 1967 and in 1971, the years immediately preceding pre- and post-amendment enforcement, respectively. The effect of enforcement on earnings in a later year is computed by subtracting this coefficient from that of the later year, estimated by equation (1).[7]

The effects of pre- and post-amendment enforcement show differences in timing but similarity in direction, with the exception of pre-amendment settlements. Post-amendment investigations and settlements both increase the earnings of white females and neither decreases the earnings of white males (Table 1); the effect of investigations begins immediately and grows over time, while the effect of settlements appears in 1974. Pre-amendment as well as

Table 1

Estimated Effect of Enforcement of Sex Discrimination Charges Under Title VII on the Earnings of White Females and Males, Unrestricted Version - Equation (1), 1968-1974

Period of enforcement, equation, and year	Earnings							
	Females				Males			
	Investigations		Settlements		Investigations		Settlements	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-amendment enforcement (1968-1972)								
1967 Equation	-.020	...	.131	...	-.024	...	.143	...
	(0.33)		(5.04)		(0.59)		(7.91)	
Equation (1)								
1968	...	-.048	...	.193	...	-.010	...	.129
		(1.01)		(11.57)		(0.31)		(10.11)
1969	...	.047	...	.129	...	.096	...	.081
		(0.99)		(7.70)		(2.81)		(6.14)
1970	...	-.001	...	.156	...	.033	...	.080
		(0.03)		(9.57)		(0.97)		(6.20)
1971	...	.104	...	.110	...	.087	...	.081
		(2.15)		(6.46)		(2.49)		(6.11)
1972	...	.178	...	.098	...	.010	...	.116
		(3.33)		(5.42)		(0.26)		(8.22)
1973	...	.134	...	.129	...	.040	...	.077
		(2.50)		(7.20)		(1.01)		(5.42)
1974	...	.242	...	.085	...	.089	...	.043
		(4.60)		(4.72)		(2.25)		(3.01)

Table 1--Continued

Period of enforcement, equation, and year	Earnings							
	Females				Males			
	Investigations		Settlements		Investigations		Settlements	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-amendment enforcement (1972-1974)								
1971 Equation	-.151	...	.015	...	-.064	...	-.016	...
	(7.06)		(0.59)		(4.09)		(0.75)	
Equation (1)								
1972	...	-.079	...	-.015		-.055		-.034
		(4.12)		(0.65)		(3.90)		(1.83)
1973	...	-.080	...	-.006	...	-.030	...	-.013
		(4.20)		(0.26)		(2.09)		(0.69)
1974	...	-.043	...	.055	...	-.028	...	-.043
		(2.28)		(2.45)		(2.00)		(2.27)

Source: U.S. Bureau of the Census, Current Population Survey, 1968-1975, computer tapes; U.S. Equal Employment Opportunity Commission, computer tapes of compliance data; and U.S. Department of Labor, Employment and Training Report of the President, 1977.

Note: t-statistics are in parentheses. The set of numbers in the following pairs of columns are from the same equation: (1) and (3), (2) and (4), (5) and (7), and (6) and (8).

post-amendment investigations increase female earnings; by contrast, however, the effect appears only with a lag in 1971. From then on, fairly steady growth results in a sizable positive long-run impact by 1974 (see Table 1, columns 1 and 2). In contrast, the coefficient of pre-amendment investigations on male earnings fluctuates from year to year, showing no clear effect (Table 1, columns 5 and 6).

Finding the positive effect of pre-amendment investigations replicated by post-amendment investigations is especially noteworthy because they cover different time periods and rank states differently. And not only is post-amendment enforcement widespread in different states, but also in ones with worse relative earnings positions for women. While pre-amendment investigations are unrelated either to earnings or to relative earnings in 1967, post-amendment investigations are negatively related to both of them in 1971 (Table 1, columns 1 and 5). Therefore, replicating results--increases in female earnings with more Title VII investigations--especially in worse states, means that our enforcement measures, although they vary only across states (within each sector), are capturing more than pure state effects that could be wiped out by a set of state dummies.

Surprisingly, pre-amendment settlements have a large, significant negative effect on the earnings of both sexes. On male earnings, that effect begins in 1969 and grows larger through 1974 (columns 7 and 8). Initially in 1968, pre-amendment settlements actually increase female earnings, but this effect dissipates by 1969; and in 1971, it begins a negative trend which continues through 1974 (columns 3 and 4). Explaining this negative long-run effect of settlements on earnings, particularly female earnings, is a primary goal of this paper. Is it an inevitable consequence of

enforcement itself or a consequence of worsening economic conditions during the early 1970s?

This question is answered by examining estimates of equations (2) and (3), which both restrict the effect of Title VII to be a linear function of the inverse of the aggregate unemployment rate. The estimates in equation (2) of α_1, β_1, and β_2 appear in the Appendix, Table A.1. The estimates of coefficients on the enforcement variables and the unemployment rate from equation (3) appear in Table 2, and of the complete equations, in the Appendix, Table A.2. Because the unemployment rate was the same in 1974 as in 1972, collinearity permits us to test the unemployment interaction hypothesis only for pre-amendment enforcement.

To test H_0 against H_1, whether the unemployment rate accounts for <u>all</u> of the year to year differences in the effect of enforcement on earnings, equation (2) is compared with equation (1) using an F-test. While the null hypothesis H_0 cannot be rejected at a significance level of 0.2 percent, we can reject, at a significance level of 5 percent for males and 1 percent for females, the null hypothesis H_0^* that the aggregate unemployment rate accounts for <u>none</u> of the year to year differences in the effect of pre-amendment Title VII on earnings. Most of pre-amendment settlements' negative effect on earnings is explained by variation in the aggregate unemployment rate. As hypothesized, the relationship is inverse (Table 2, columns 3 and 6). The positive coefficients on PREENF *1/UNEMPL imply that increasing the unemployment rate makes the effect of settlements less positive (or more negative). Its effect on female earnings, .0048 (1/UNEMPL), is twice as sensitive to the inverse of the unemployment rate as its effect on male earnings, .0024 (1/UNEMPL).[8]

Table 2

Estimated Effect of Enforcement of Sex Discrimination Charges Under Title VII on the Earnings of White Females and Males, Restricted Version - Equation (3), 1968-1974

Years in equation and independent variable	Earnings			
	Females		Males	
1968-1974				
Aggregate unemployment rate/100 (UNEMPL)	-11.476 (7.62)		-16.108 (13.24)	
	Investigations	Settlements	Investigations	Settlements
Pre-amendment enforcement (PREENF)	.055 (0.33)	.027 (0.88)	.114 (0.92)	.038 (1.57)
PREENF * 1/UNEMPL	-.004 (0.68)	.0048 (3.50)**	-.003 (0.59)	.0024 (2.21)*
PREENF * TIME1	.030 (2.40)**	-.0003 (1.21)	-.002 (0.26)	-.0002 (1.06)
1972-1974				
Post-amendment enforcement (POSTENF)	-.071 (6.45)	-.066 (2.14)	-.041 (4.95)	.012 (0.47)
POSTENF * 1/UNEMPL	...	...	...	...
POSTENF * TIME2	.0015 (2.46)**	.035 (2.51)**	.0011 (2.57)**	-.021 (1.79)
R^2	.4043		.5392	
S.E.	.6446		.5641	

Source: Same as Table 1.

Note: t-statistics are in parentheses.

*For interaction variables, indicates significance at the 5 percent level using a one-tail test.

**For interaction variables, indicates significance at the 1 percent level using a one-tail test.

Although independent of the unemployment rate, the effect of pre-amendment investigations on female earnings depends positively, as hypothesized, upon time. Likewise, the effects of post-amendment investigations and settlements on female earnings and of post-amendment investigations on male earnings do as well. On female earnings, pre-amendment investigations' effect trends upward at 3 percent per year (Table 2, column 1), and post-amendment settlements' effect, at 3.5 percent (Table 2, column 3). The effects of post-amendment investigations trend upward more slowly, on female earnings at .15 percent per year (Table 2, column 1) and on male earnings at .11 percent (Table 2, column 4).[9]

The relatively lower frequency of pre-amendment settlements explains why they differ from pre-amendment investigations in their interaction with time. As noted above, firms use sample information to gradually alter their prior expectations about enforcement. Pre-amendment settlements, relatively infrequent, provided inadequate information to change firms' expectations. But post-amendment settlements, more widespread, were also more of a threat. The 1972 amendments to Title VII had given the EEOC the power to litigate cases. Information about the ensuing court cases was adequate to alter firms' expectations and to impart the positive time trend visible in the post-amendment effects of settlements.[10]

The underlying model illuminates why investigations and settlements differ in their interaction with the unemployment rate. By providing firms with information about enforcement activities, investigations deter employment and wage discrimination. If the information causes firms to change employment practices, their expected marginal costs of violating the

law must exceed their marginal costs of complying with it. Then holding risk preferences constant, small changes in the latter would not cause observable changes in their practices. Changing the unemployment rate causes such small changes; hence, the effect of investigations on earnings would be independent of the unemployment rate.

Some firms violate the law despite that information. Individuals file charges against them, leading to conciliated or litigated settlements. For these firms, the expected marginal costs of violating the law must be less than or equal to their marginal costs of complying with it. Then, small increases in the latter, induced by increases in the unemployment rate, create even greater inequality in marginal costs, causing firms to react. Therefore, the outcomes of and extent of adherence to settlements, as well as their effect on earnings, should depend upon the unemployment rate. The proportion of settlements that the EEOC successfully conciliates is one indicator. And data show that as the average unemployment rate increased from 4.475 to 5.367 percent between the pre- and post-amendment periods, the proportion of all private sector settlements conciliated successfully dropped from .646 to .577 (Beller, forthcoming).

4. THE EFFECT OF TITLE VII AT ALTERNATIVE UNEMPLOYMENT RATES

In order to see how the effectiveness of Title VII was restricted by its sensitivity to the business cycle, we have computed the effects of pre-amendment settlements and of Title VII at the actual and at several alternative unemployment rates. Table 3 presents these effects for both the short-run

Table 3

Estimated Impact of Pre-Amendment Successful Settlements of Sex Discrimination Charges Under Title VII and of All Enforcement Measures on the Earnings of White Females and Males, at Alternative Unemployment Rates, 1967-1974

Period and unemployment rate	Unemployment Rate (1)	Pre-Amendment settlements: Earnings Females (2)	Pre-Amendment settlements: Earnings Males (3)	Pre-Amendment settlements: Earnings Differential (4)	All enforcement measures: Earnings Females (5)	All enforcement measures: Earnings Males (6)	All enforcement measures: Earnings Differential (7)
Short-run, 1967-1971							
Actual 1971 unemployment rate	5.9%	-.021	-.072	-.051	+.003	-.028	-.031
Plus 1.0 percent	6.9	-.033	-.077	-.044	-.002	-.030	-.028
Minus 1.0 percent	4.9	-.003	-.065	-.062	+.011	-.025	-.036
1969 unemployment rate	3.5	+.038	-.049	-.086	+.028	-.020	-.048
Long-run, 1967-1974							
Actual 1974 unemployment rate	5.6	-.017	-.071	-.054	+.057	-.039	-.096
Plus 1.0 percent	6.6	-.030	-.076	-.046	+.051	-.041	-.092
Minus 1.0 percent	4.6	+.003	-.063	-.066	+.065	-.037	-.102
1969 unemployment rate	3.5	+.037	-.049	-.086	+.079	-.032	-.111

Source: Tables 1 and 2.

(1967-1971) and the long-run (1967-1974). Settlements' effects (columns 2-4) are computed as the difference between the function in the later year, taken from Table 2, and the coefficient in the pre-enforcement year, taken from line 1 of Table 1. The short-run effect of Title VII (all enforcement measures, columns 5-7) sums the short-run coefficient differences of pre-amendment settlements and investigations, evaluated at their respective means; similarly, the long-run effect sums the long-run coefficient differences of pre-amendment settlements and investigations as well as the short-run coefficient differences of post-amendment settlements and investigations.

The long-run negative effect that pre-amendment settlements had on female earnings is a consequence not of enforcement itself, but rather of worsening economic conditions following those settlements. Had the unemployment rate been but one percentage point lower, 4.6 percent in 1974, pre-amendment settlements would have increased not reduced female earnings in the long run (Table 3, column 2, line 7). And at the 1969 unemployment rate of 3.5 percent, an increase of 0.3 in the pre-amendment probability of successful settlement would have increased women's earnings by 1.2 percent instead of reducing them 0.6 percent. Male earnings would also have been reduced by about 0.6 percent less and the earnings differential narrowed by 1.0 percent more.

Actually, enforcement of Title VII left the earnings of white females unchanged by 1971, but increased them 5.7 percent by 1974 (Table 3, column 5). While the increase in the unemployment rate eroded any gains that women may have made from pre-amendment enforcement, the higher rate of the

early 1970s did not preclude new gains. How much larger they might have been, we cannot tell. Evidently though, the factors (the 1972 amendments, legal developments and administrative rulings, possibly a reduction in discrimination against women) making post-amendment Title VII more effective outweighed the perverse effects of a higher, albeit nonincreasing, unemployment rate during the early 1970s. Even so, women's earnings could have been higher in 1974, and the sex differential smaller had the unemployment rate been lower.

How much higher women's earnings could have been is estimated at the 1969 unemployment rate of 3.5 percent. The lowest rate in recent years, its choice allows us to reasonably estimate the potential effects of Title VII. Had that unemployment rate been maintained, Title VII would have increased female earnings by close to 3 percent in the short-run and 8 percent in the long-run, making them 2.5 percent higher than they were in 1971 and 2.2 percent higher than in 1974. Title VII would also have reduced male earnings by nearly 1 percent less than it did. The potential of Title VII for narrowing the sex differential was 4.8 percent in the short-run and 11.1 percent in the long-run, a full 55 percent and 16 percent more than the actual narrowing of 3.1 and 9.6 percent, respectively. The real earnings cost to narrowing the differential by 11.1 percent would have been only 3.2 percent in male earnings, balanced against a 7.9 percent gain in female earnings (Table 3, line 8). This tradeoff contrasts favorably to the actual one, a real earnings loss of 3.9 percent for males and gain of 5.7 percent for females.[11] At its potential, Title VII is a relatively costless tool for narrowing the sex differential in earnings.[12]

5. CONCLUSION

The increase in the unemployment rate during the early 1970s substantially curtailed the effectiveness of Title VII. It interacted with pre-amendment Title VII settlements to limit the increase in female earnings, exaggerate the reduction in male earnings, and restrict the narrowing of the earnings differential. Nevertheless, even at high unemployment rates, Title VII still narrowed the sex differential in earnings. But the real earnings cost was greater and the earnings redistribution less than at a lower unemployment rate.

The conventional wisdom has proved correct: The recession of the 1970s eroded gains made from Title VII of the Civil Rights Act of 1964. To what extent the effects of other social programs, and our estimates of them, have also been affected by the recession is unknown, and worth investigation.

Appendix

Table A.1

Estimated Effect of Enforcement of Sex Discrimination Charges Under Title VII on the Earnings of White Females and Males, Restricted Version - Equation (2), 1968-1974

Years in equation and independent variable	Earnings			
	Females		Males	
1968-1974				
Aggregate unemployment rate/100 (UNEMPL)	-11.718 (7.74)		-15.953 (13.06)	
	Investigations	Settlements	Investigations	Settlements
Pre-amendment enforcement (PREENF)	.391 (4.36)	.024 (0.77)	.084 (1.29)	.037 (1.55)
PREENF * 1/UNEMPL	-.014 (3.51)	.005 (3.57)	-.002 (0.55)	.002 (2.21)
1972-1974				
Post-amendment enforcement (POSTENF)	.070 (0.45)	.102 (0.56)	-.115 (0.98)	-.196 (1.28)
POSTENF * 1/UNEMPL	-.007 (0.85)	-.005 (0.54)	.004 (0.64)	.009 (1.08)
R^2	.4043		.5392	
S.E.	.6446		.5641	

Source: Same as Table 1.

Note: t-statistics are in parentheses.

Table A.2

Equation (3)--Restricted

Independent variables	Females		Males	
	Coefficient	(t-value)	Coefficient	(t-value)
Constant	3.908	(52.58)	4.891	(81.64)
South	-.088	(16.25)	-.066	(16.21)
North Central	-.061	(11.34)	.039	(9.78)
West	-.068	(11.47)	-.019	(4.35)
SMSA	.160	(42.77)	.139	(49.05)
Government	.188	(26.26)	-.019	(3.20)
Local unemployment rate	1.086	(7.13)	1.060	(9.24)
Single	-.154	(29.22)	-.426	(102.50)
Other married	-.009	(1.91)	-.175	(30.43)
Veteran	N.A.	N.A.	.041	(14.24)
Number of children	-.032	(20.59)	N.A.	N.A.
Home	-.145	(29.07)	N.A.	N.A.
Health	N.A.	N.A.	-.058	(8.24)
Education = X_1	.079	(49.90)	.073	(70.61)
Experience = X_2	.030	(31.10)	.046	(54.74)
$(\text{Experience})^2 = X_3$	-.0005	(24.97)	-.0007	(47.08)
Ln (weeks worked) = X_4	-.157	(24.76)	-.237	(42.74)
Part-time = X_5	-.742	(72.51)	-.815	(70.21)
Federal share = X_6	.306	(11.25)	.210	(13.61)
1969 * X_1	-.010	(4.90)	-.005	(3.22)
1969 * X_2	-.0006	(0.42)	-.007	(5.57)

Table A.2--Continued

Independent variables	Females		Males	
	Coefficient	(t-value)	Coefficient	(t-value)
1969 * X_3	-.0000	(0.21)	.0001	(4.06)
1969 * X_4	.045	(6.07)	.044	(7.34)
1969 * X_5	-.142	(10.08)	-.146	(9.08)
1969 * X_6	.069	(1.75)	-.0001	(0.01)
1970 * X_1	-.003	(1.26)	-.004	(2.87)
1970 * X_2	-.002	(1.20)	-.006	(5.18)
1970 * X_3	.0000	(0.75)	.0001	(3.48)
1970 * X_4	.068	(8.62)	.097	(14.63)
1970 * X_5	-.144	(10.33)	-.111	(6.96)
1970 * X_6	.016	(0.40)	.014	(0.62)
1971 * X_1	.001	(0.64)	-.004	(2.83)
1971 * X_2	-.0003	(0.22)	-.002	(2.00)
1971 * X_3	.0000	(0.43)	.0000	(0.72)
1971 * X_4	.075	(8.46)	.125	(15.62)
1971 * X_5	-.125	(8.72)	-.080	(4.91)
1971 * X_6	.080	(1.92)	.006	(0.24)
1972 * X_1	-.005	(2.04)	-.005	(3.55)
1972 * X_2	.002	(1.19)	-.003	(2.96)
1972 * X_3	-.0001	(1.79)	.0000	(1.55)
1972 * X_4	.102	(11.73)	.134	(17.43)
1972 * X_5	-.132	(9.21)	-.064	(3.95)
1972 * X_6	.088	(2.15)	.006	(0.24)
1973 * X_1	-.006	(3.05)	-.006	(4.19)
1973 * X_2	.0009	(0.71)	-.003	(2.19)
1973 * X_3	-.0000	(1.32)	.0000	(0.52)

Table A.2--Continued

Independent variables	Females		Males	
	Coefficient	(t-value)	Coefficient	(t-value)
1973 * X_4	.080	(9.94)	.113	(16.71)
1973 * X_5	-.136	(9.78)	-.078	(4.89)
1973 * X_6	.109	(2.61)	.019	(0.81)
1974 * X_1	-.003	(1.13)	-.007	(4.70)
1974 * X_2	.002	(1.26)	-.0007	(0.63)
1974 * X_3	-.0000	(1.51)	-.0000	(1.01)
1974 * X_4	.066	(7.62)	.128	(16.42)
1974 * X_5	-.122	(8.67)	-.065	(4.01)
1974 * X_6	.080	(1.95)	.040	(1.66)
Aggregate unemployment rate ÷ 100 (UNEMPL)	-11.476	(7.62)	-16.108	(13.24)
Investigations (1968-72) = E_1	.055	(0.33)	.114	(0.92)
(1/UNEMPL) * E_1	-.004	(0.68)	-.003	(0.59)
TIME 1 * E_1	.030	(2.40)	-.002	(0.25)
Settlements (1968-72) = E_2	.027	(0.88)	.038	(1.57)
(1/UNEMPL) * E_2	.005	(3.50)	.002	(2.21)
TIME 1 * E_2	-.0003	(1.21)	-.0002	(1.06)
Investigations (1972-74) = E_3	-.071	(6.45)	-.041	(4.95)
TIME 2 * E_3	.001	(2.46)	.001	(2.57)
Settlements (1972-74) = E_4	-.066	(2.14)	.012	(0.47)
TIME 2 * E_4	.035	(2.51)	-.021	(1.79)
R^2	.4043		.5392	
S.E.	.6446		.5641	
Number of observations	201,623		147,437	

Source: Same as Table 1.

N.A. = not applicable.

NOTES

[1]This paper focuses on earnings in order to reexamine earlier findings. EEO laws are expected to affect employment and occupational distributions as well, and research into these effects is currently underway.

[2]According to the Heckman-Wolpin model, in the long run, an employment quota increases the firm's costs of production and (a sex quota) reduces the employment of men, and possibly of women, below the pre-quota equilibrium. The reduction in employment tends to reduce their wages.

[3]Some economists have argued that the recession of the 1970s eroded gains minorities made due to the social legislation of the 1960s (see, e.g., Wallace, 1976).

[4]The costs of violating the law, which are weighted by the probabilities to determine expected costs, may include lawyer's fees and court costs, large back-pay settlements, and substantial changes in hiring and other personnel practices. This study assumes they are constant across firms and over time for a specific violation. The 1972 amendments to Title VII increased the probability that a firm found violating the law would pay any of these costs; they did so by granting the Equal Employment Opportunity Commission (EEOC) the right to sue private sector respondents if conciliation attempts failed.

[5]If these constraints did not hold, the results would be biased toward finding no relationship between the effect of enforcement and the unemployment rate; hence, we adopt them for computational convenience.

[6]The sample is restricted to whites only to reduce the size of the cross-products matrix by eliminating race interaction terms.

[7]Computing the effect of enforcement as a coefficient difference nets out any preexisting relationship between variation in enforcement and in earnings across states.

[8]Factors other than the increasing unemployment rate during the early 1970s, such as deteriorating terms of trade or energy problems, may also have caused real earnings to fall and could affect our estimates of the earnings losses accompanying the early Title VII settlements. To test this, we estimated r-stricted equation (3), allowing a different intercept in each year. This model did not alter the estimated effect of enforcement or its interaction with the unemployment rate.

[9]The effect of post-amendment settlements on male earnings has a negative time trend, but is insignificant at the 5 percent level; because the period is short, we place little confidence in a coefficient significant at less than 5 percent.

[10]Since both post-amendment enforcement measures show a positive time trend, discrimination against women, hence the costs of complying with Title VII, may have decreased between 1972 and 1974.

[11]We can make statements about gains and losses in earnings, but not in welfare. To do so, we would need to know the impact of Title VII on employment, which is difficult to estimate for intercensal years. It could also be misleading to make strong time-series statements from these primarily cross-section results. Nevertheless, by contrasting post-amendment

with pre-amendment enforcement our results provide some idea, at least about direction, of the change in Title VII's effects over time.

[12]Ironically, an increase in the unemployment rate causes the male/female earnings differential to narrow as well; this occurs because male earnings are more cyclically sensitive than female earnings. The 2 percentage point increase in the unemployment rate between 1968 and 1974, from 3.6 to 5.6 percent, caused the sex differential to narrow by 8 percent.

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